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Working Memory: Theories, Models, and Controversies

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Keywords

attention, central executive, phonological loop, episodic buffer, visuo-spatial sketchpad, short-term memory

Abstract

I present an account of the origins and development of the multicomponent approach to working memory, making a distinction between the overall theoretical framework, which has remained relatively stable, and the attempts to build more specific models within this framework. I follow this with a brief discussion of alternative models and their relationship to the framework. I conclude with speculations on further developments and a comment on the value of attempting to apply models and theories beyond the laboratory studies on which they are typically based.

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WORKING MEMORY: THEORIES, MODELS, AND CONTROVERSIES

WM: working memory

M-WM: multicomponent working memory

I was honored, pleased, and challenged by the invitation to write this prefatory chapter, pleased because it offered the chance to take a broad and somewhat autobiographical view of my principal area of interest, working memory (WM), but challenged by the potential magnitude of the task. The topic of working memory has increased dramatically in citation counts since the early years, not all of course related to or supportive of my own work, but a recent attempt to review it (Baddeley 2007) ended with more than 50 pages of references. What follows is a partial, as opposed to impartial, account of the origins of the concept of multicomponent working memory (M-WM) and of my own views on its subsequent development. My first draft would have filled the chapter page allowance with references; I apologize to all of those whose work should have been cited and is not.

I entered psychology as a student at University College London in 1953, a very exciting time for the field of psychology, which had benefited greatly from developments during the Second World War, where theory was enriched by the need to tackle practical problems. As a result, prewar issues such as the conflict between Gestalt psychology and neobehaviorism began to be challenged by new data and new ideas, some based on cybernetics, the study of control systems, with others influenced by the newly developed digital computers. This in turn led to a renewed interest in the philosophy of science as applied to psychology. Typical questions included, is psychology a science?; if so, is it cumulative or are we doomed to keep on asking the same questions, as appeared to be the case in philosophy? What would a good psychological theory look like?

As students we were offered two answers to this question. The first, championed by Cambridge philosopher Richard Braithwaite (1953), regarded Newton's Principia as the model to which scientific theories should aspire, involving as it does postulates, laws, equations, and predictions. Within psychology, the Newtonian model was explicitly copied by Clark Hull in his attempt to produce a general theory of learning, principally based on the study of maze learning in the albino rat.

An alternative model of theorizing came from Oxford, where Stephen Toulmin (1953) argued that theories were like maps, ways of organizing our existing knowledge of the world, providing tools both for interacting with the world and for further exploration. Edward Tolman in Stanford had a view of learning in rats that fitted this model, using it to challenge Hull's neo-behaviorist approach. This raised the crucial question as to how you might decide between the two apparently opposing views. The dominant answer to that question, in the United Kingdom at least, was provided by Karl Popper (1959), a Viennese-trained philosopher who argued strongly that a valid theory should make clear, testable predictions, allowing the rival theories to confront each other in the all-important "crucial experiment" that settles the issue. This approach was closer in spirit to Hull than to Tolman.

My own first published study (Baddeley 1960) attempted just such a crucial experiment, predicting that rats would be smarter than they should be according to Hullian theory, and demonstrating, to my own satisfaction at least, that this was the case. Alas, by the time it was published, the whole field of learning theory seemed to have collapsed. Neither side was able to deliver a knockout blow, and people simply abandoned the research area. I resolved at that point that if I myself were to develop a theory, it would be based very closely on the evidence, which would survive even if the theory proved totally wrong. It is an approach I have followed ever since.

But what is the answer to our original question, should theorists be architects, building elegant structures such as Newton did, or should they be explorers, gradually extending the theory on the basis of more and more evidence, as in the case of Darwin? Clearly both Newton and Darwin got it right, but for fields at a different stage of development. Newton claimed that his success resulted from "standing on the shoulders of giants," who no doubt stood on the shoulders of lesser mortals like ourselves. Darwin had few such giants available. I suggest that any complete theory is likely to require explorers in its initial stages and architects to turn the broad concepts into detailed models. I myself am very much at the explorer end of the continuum, but I fully accept the importance of the skills of the architect if theory is to develop.

My research career really began with my arrival at the Medical Research Council Applied Psychology Unit (APU) in Cambridge. Its role was to form a bridge between psychological theory and practical problems, and the year I arrived, Donald Broadbent, its director, had just published his seminal book, *Perception and Communication*, which provided one of the sparks that ignited what subsequently became known as the cognitive revolution. I was assigned to work on optimizing the design of postal codes,

STM: short-term memory

LTM: long-term memory

which led me to combine the classic tradition of nonsense syllable learning with new ideas from information theory, resulting in my generating memorable postal codes for each town in the United Kingdom. The Post Office thanked me and went on their way regardless; the code they adopted could, however, have been much worse, as is indeed the case in some countries, but that is another story.

By this time my approach to theory was evolving away from Popper's idea of the need for crucial experiments, largely on the grounds that clear predictions only appeared to be possible in situations that were far narrower than the ones I found interesting. I subsequently discovered that within the philosophy of science, Lakatos (1976), and allegedly Popper himself, had subsequently abandoned the reliance on falsification, arguing instead that the mark of a good theory is that it should be productive, not only giving an account of existing knowledge, but also generating fruitful questions that will increase our knowledge. This more map-like view of theory is the one that I continue to take.

Short-Term Memory

The term "working memory" evolved from the earlier concept of short-term memory (STM), and the two are still on occasion used interchangeably. I will use STM to refer to the simple temporary storage of information, in contrast to WM, which implies a combination of storage and manipulation.

My interest in STM began during my time at the APU in Cambridge and was prompted by an applied problem, that of finding a way of evaluating the quality of telephone lines that might be more effective than a simple listening test. My PhD supervisor Conrad had recently discovered the acoustic similarity effect. He was studying memory for proposed telephone dialing codes when he noted that even with visual presentation, memory errors resembled acoustic mis-hearing errors (e.g., *v* for *b*), and that memory for similar sequences (*b g t p c*) was poorer than for dissimilar (*k r l q y*), concluding that STM depends on an acoustic code (Conrad & Hull 1964).

I decided to see if the acoustic similarity effect could be used to provide sensitive indirect measure of telephone line quality. It did not; the effects of noise and similarity were simply additive, but I was intrigued by the sheer magnitude of the similarity effect. Similarity was a central variable within the dominant stimulus-response interference theory of verbal learning (see Osgood 1949), but the type of similarity seemed not to be regarded as important. So, would Conrad's effect generalize to other types of similarity in STM?

I tested this, comparing recall of sequences with five phonologically similar words (*man*, *mat*, *can*, *map*, *cat*), five dissimilar words (e.g., *pit*, *day*, *cow*, *pen*, *sup*), and five semantically similar sequences (*huge*, *big*, *wide*, *large*, *tall*) with five dissimilar (*wet*, *soft*, *old*, *late*, *good*). I found (Baddeley 1966a) a huge effect of phonological similarity[1] (80% sequences correct for dissimilar, 10% for similar) and a small but significant effect for semantic similarity (71% versus 65%). I went on to demonstrate that this pattern reversed when long-term memory (LTM) was required by using ten-word lists and several learning trials; semantic similarity then proved critical (Baddeley 1966b). I concluded that there were two storage systems, a short-term phonological and a long-term semantically based system. My telephony project was passed on to a newly arrived colleague and I was left free to explore this line of basic research.

I saw my work as fitting into a pattern of evidence for separate STM and LTM stores. Other evidence came from amnesic patients who had preserved STM and impaired LTM, while other patients showed the reverse pattern (Shallice & Warrington 1970). A third source of evidence came from two-component memory tasks, which comprised a durable LTM component together with a temporary component. A typical example of this was the recency effect

[1]I subsequently abandoned the term "acoustic similarity" because it suggested an input modality-based system, which is not the case; I mistakenly assumed that phonological was a more neutral term. It was not intended as a statement of the linguistic basis of the memory system, which remains an open question.

in free recall (Glanzer 1972); the last few words of a list are well recalled on immediate test but not after a brief filled delay, unlike earlier items.

At this point, my simple assumption of two stores, with STM phonologically based and LTM semantically based, led to some clear predictions. Amnesic patients should have semantic coding problems, and recency should be acoustically based. Studies based on amnesic patients suffering from Korsakoff's syndrome did suggest a semantic encoding deficit (Cermak et al. 1974), but our own work showed no evidence of such a deficit (Baddeley & Warrington 1970), and later work (Cermak & Reale 1978) attributed their previously observed deficit to additional executive problems, often found in Korsakoff's syndrome.

In the case of two-component tasks, it became clear that recency did not depend on verbal STM (Baddeley & Hitch 1977) and that the use of semantic or phonological coding was strategy dependent. Phonological coding of verbal material is rapid, attentionally undemanding, and very effective for storing serial order. Semantic coding can be rapid for meaningful sequences such as sentences, but it is much harder to use for storing the order of unrelated words (Baddeley & Levy 1971). We also showed that word sequences can simultaneously be encoded both phonologically and semantically (Baddeley & Ecob 1970) and that standard tasks such as immediate serial recall can reflect both long-term and short-term components, each of which may be influenced by either phonological or semantic factors. In short, STM, retention of material over a brief period, may be based on either phonological or semantic coding. The former is easy to set up but readily forgotten; the latter may take longer to set up but tends to be more durable. Both can operate over brief delays, and the fact that we can learn new words indicates that long-term phonological learning also occurs.

It is worth emphasizing the need to distinguish between STM as a label for a paradigm in which small amounts of information are stored over brief delays and STM as a theoretical storage system. This point was made by Waugh & Norman (1965) and by Atkinson & Shiffrin (1968), but it has often been neglected in subsequent years. Material tested after a brief delay (i.e., an STM task) is likely to reflect both LTM and some form of temporary storage.

Evolution of a Multicomponent Theory

After nine years at the APU, I moved to Sussex into a new department of experimental psychology, where, in 1972, I was joined by Graham Hitch as a post-doctoral fellow on my first research grant. After a first degree in physics, he had done a psychology MSc in Sussex and a PhD with Broadbent at the APU. We had proposed (perhaps unwisely) to investigate the link between STM and LTM, beginning our grant just when the previously popular field of STM was downsizing itself following criticism of the dominant Atkinson & Shiffrin (1968) model for three reasons. First, the model assumed that merely holding information in STM would guarantee transfer to LTM, whereas Craik & Lockhart (1972) showed that the nature of processing is crucial, with deeper, more elaborate processing leading to better learning. Second, its assumption that the short-term store was essential for access to LTM proved to be inconsistent with neuropsychological evidence. Patients with a digit span of only two items and an absence of recency in free recall should, according to Atkinson and Shiffrin, have a defective short-term store that should lead to impaired LTM. This was not the case. Third, given that Atkinson and Shiffrin assumed their short-term store to be a working memory, playing an important general role in cognition, such patients should have major intellectual deficits. They did not. One patient, for instance, was an efficient secretary, and another ran a shop and a family. Interest in the field began to move from STM to LTM, to semantic memory and levels of processing.

Graham Hitch and I did not have access to these rare but theoretically important STM-deficit patients and instead decided that we would try to manufacture our own "patients" using student volunteers. We did

CE: central executive

so, not by removing the relevant part of their brain, but by functionally disabling it by requiring participants to do a concurrent task that was likely to occupy the limited-capacity short-term storage system to varying degrees. The concurrent task we chose was serial verbal recall of sequences of spoken digits. As sequence length increased, the digits should occupy more and more of available capacity, with the result that performance on any task relying on WM should be progressively impaired. In one study, participants performed a visually presented grammatical reasoning task while hearing and attempting to recall digit sequences of varying length. Response time increased linearly with concurrent digit load. However, the disruption was far from catastrophic: around 50% for the heaviest load, and perhaps more strikingly, the error rate remained constant at around 5%. Our results therefore suggested a clear involvement of whatever system underpins digit span, but not a crucial one. Performance slows systematically but does not break down. We found broadly similar results in studies investigating both verbal LTM and language comprehension, and on the basis of these, abandoned the assumption that WM comprised a single unitary store, proposing instead the three-component system shown in **Figure 1** (Baddeley & Hitch 1974).

We aimed to keep our proposed system as simple as possible, but at the same time, potentially capable of being applied across a wide range of cognitive activities. We decided to split attentional control from temporary storage, which earlier research suggested might rely on separate verbal and visuo-spatial short-term systems, all of which were limited in capacity. We labeled the central controller as a "central executive" (CE), initially referring to the verbal system as the "articulatory loop," after the subvocal rehearsal assumed to be necessary to maintain information, and later adopting the term "phonological loop" to emphasize storage rather than rehearsal. We termed the third component the "visuo-spatial sketchpad," leaving open the issue of whether it was basically visual, spatial, or both.

We began by focusing on the phonological loop on the grounds that it seemed the most tractable system to investigate, given the very extensive earlier research on verbal STM. At this point, I unexpectedly received an invitation from Gordon Bower to contribute a chapter to an influential annual publication presenting recent advances in the area of learning and memory. We hesitated; our model was far from complete, should we perhaps wait? We went ahead anyhow (Baddeley & Hitch 1974), presenting a model that is still not complete nearly 40 years and many publications later.

Over the next decade we continued to explore the model and its potential for application beyond the cognitive laboratory. At this point I agreed to summarize our progress in a monograph (Baddeley 1986). This was approaching completion when I realized that I had said nothing about the CE, very much a case of Hamlet without the prince. My reluctance to tackle the executive stemmed from two sources: first, its probable complexity, and second, because of the crucial importance of its attentional capacity. Although there were a number of highly developed and sophisticated theories of attention, most were concerned with the role of attention in perception, whereas the principal role of the CE was the attentional control of action. The one directly relevant article I could find (Norman & Shallice 1986) appeared as a chapter because of the difficulty of persuading a journal to accept it (Shallice 2010, personal communication), alas, all too common with papers presenting new ideas.

Norman and Shallice proposed that action is controlled in two rather separate ways. One is

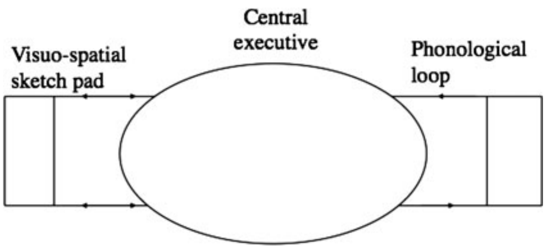

Figure 1
The original Baddeley & Hitch (1974) working memory model.

based on well-learned habits or schemata, demanding little in the way of attentional control. An example of this might be the activity of driving a well-learned route to your office. This source of control can be overridden by a second process, the supervisory attentional system (SAS), which responds to situations that are not capable of being handled by habit-based processes, for example, coping with the closure of a road on your normal route.

With some relief, I incorporated the Norman and Shallice model into my own concept of a CE, producing a book (Baddeley 1986) that attempted to pull together developments in WM that had occurred in the previous decade and then apply them to data from the literature in three areas: fluent reading, the development of WM in children, and the effects of aging. Although I tended to refer to our proposals as a model, using the criteria proposed earlier, it might better be regarded as a simple theory, in the sense of Toulmin's idea of theories as maps, linking together existing knowledge and encouraging further investigation. If so, it was a map with many blank areas that I hoped would be filled by myself and others, leading in due course to more detailed modeling.

What then are the essentials of the broad theory? The basis is the assumption that it is useful to postulate a hypothetical limited-capacity system that provides the temporary storage and manipulation of information that is necessary for performing a wide range of cognitive activities. A second assumption is that this system is not unitary but can be split into an executive component and at least two temporary storage systems, one concerning speech and sound while the other is visuo-spatial. These three components could be regarded as modules in the sense that they comprise processes and storage systems that are tightly interlinked within the module and more loosely linked across modules, with somewhat more remote connections to other systems such as perception and LTM. I regard the very rigid definition of modularity by Fodor (1983) as unhelpful and neuropsychologically implausible. A consequence of my rejection of Fodorian simplicity is the assumption that each of these systems can be fractionated into subsystems and that these will be linked to perceptual and LTM processes in ways that require further investigation.

My overall view of WM therefore comprised, and still comprises, a relatively loose theoretical framework rather than a precise model that allows specific predictions. The success of such a framework should be based, as suggested by Lakatos (1976), not only on its capacity to explain existing data but also on its productivity in generating good, tractable questions linked to empirical methods that can be widely applied. The proposed components of WM are discussed in turn, beginning with the phonological loop.

CHARACTERISTICS OF THE PHONOLOGICAL LOOP

We saw the phonological loop as a relatively modular system comprising a brief store together with a means of maintaining information by vocal or subvocal rehearsal. In the 1960s, a number of studies attempted to decide whether forgetting in the STM system was based on trace decay or interference (see Baddeley 1976). None of these studies proved to be conclusive, a state of affairs that remains true, in my own opinion. We opted to assume a process of trace decay, partly on the basis of our results and partly because it avoided the need to become involved in the many controversies surrounding traditional approaches to interference theory at the time (see Baddeley 1976, chapter 5), although we did assume a limited-capacity store, which in turn implies some unspecified form of interference, either by displacement or by overwriting. We used existing results, together with our own subsequent studies, to create a simple model that is based on the method of converging operations. This involves combining evidence from a range of different phenomena, each consistent with the model, but each individually explicable in other ways. If none of the competing interpretations are able to explain the whole pattern, whereas the phonological loop model can, then this provides valuable support. This approach has the advantage of potentially producing a robust model, but it has the disadvantage of being required to confront a range of different possible alternative explanations for each individual phenomenon.

The Phonological Similarity Effect

As described above, this is regarded as an indication that phonological storage is involved. Its effect is principally on the storage of order information. Indeed, item information may be helped by similarity since it places constraints on possible responses. For this reason, studies that specifically attempt to investigate the loop tend to minimize the need to retain item information by repeatedly using the same limited set, for example, consonants. Studies using open sets, for instance, different words for each sequence, are more likely to reflect loss of item information and to show semantic and other LTM-based effects.

The Word Length Effect

We assumed that vocal or subvocal rehearsal was likely to occur in real time, with longer words taking longer and hence allowing more time for trace decay, thus leading to poorer performance. We studied the immediate recall of sequences of five words ranging in length from one syllable (e.g., *pen day hot cow tub*) to five syllables (e.g., *university*, *tuberculosis*, *opportunity*, *hippopotamus*, *refrigerator*) and found that performance declined systematically with word length. As expected, when participants were required to read out words of different lengths as rapidly as possible, there was a close correspondence between word length and articulation time. The simple way of expressing our results was to note that people are able to remember as many words as they can articulate in two seconds (Baddeley et al. 1975b).

We interpreted our data by assuming that longer words take longer to rehearse, resulting in more trace decay and poorer recall. Such decay is also likely to continue during the slower spoken recall of longer words. We presented evidence for time-based decay, which has since faced challenge and counter-challenge (see Baddeley 2007, pp. 43–49). Fortunately, however, the general hypothesis of a phonological loop will function equally well with either a decay or interference interpretation of short-term forgetting, illustrating the value of combining a broad theoretical map while leaving more detailed modeling to be decided by further experimentation.

Articulatory Suppression

If the word length effect is dependent on subvocalization, then preventing it should eliminate the effect. This is indeed the case (Baddeley et al. 1975b). When participants are required to continuously utter a single word such as "the," performance drops and is equivalent for long and short words. Suppression also removes the phonological similarity effect for visually presented materials but not when presentation is auditory (Baddeley et al. 1984). We interpret this as suggesting that spoken material gains obligatory access to the phonological store, whereas written material needs to be subvocalized if it is to register.

The claim that auditory presentation allows a phonological trace to be laid down despite suppression has recently been challenged. Jones et al. (2006) have suggested that the effect is limited to the recency component of immediate serial recall, suggesting that it is better regarded as a perceptual effect. However, although this may be true for long lists, shorter lists show an effect that operates throughout the serial position curve (Baddeley & Larsen 2007).

Irrelevant Sound Effects

Colle & Welsh (1976) required their participants to recall sequences of visually presented digits presented either in silence or accompanied by white noise or by speech in an unfamiliar language that they were told to ignore. Only the spoken material disrupted performance on the visually presented digits, an effect that was independent of the loudness of the irrelevant sound sources. Pierre Salame, a French visitor to Cambridge, and I followed up and extended

Colle's work, demonstrating that visual STM was disrupted to the same extent by irrelevant words and nonsense syllables; indeed, irrelevant digits had no more effect on digit recall than did nondigit words containing the same phonemes (e.g., *one two* replaced by *tun woo*), suggesting that interference was operating at a prelexical level. We did, however, find slightly less disruption of our monosyllabic digits from bisyllabic words than from monosyllabic words, concluding rather too hastily that this suggested that interference was dependent on phonological similarity (Salame & Baddeley 1986). Like Colle and Welsh, we suggested an interpretation in terms of some form of mnemonic masking. This proved to be something of an embarrassment when it was clearly demonstrated that irrelevant items that were phonemically similar to the remembered sequence were no more disruptive than dissimilar items (Jones & Macken 1995, Larsen et al. 2000). Unfortunately, our initial hypothesis came to be regarded as central to WM, despite our subsequent withdrawal, a salutary lesson in premature theorizing.

Meanwhile Dylan Jones and colleagues in Wales were developing a very extended program of research on irrelevant sound. They showed that STM was disrupted not only by irrelevant speech, but also by a range of other sounds, including, for example, fluctuating tones (Jones & Macken 1993). In order to account for their results they proposed the "changing state" hypothesis, whereby the crucial feature was that the irrelevant sound needed to fluctuate. Jones (1993) coupled this with the object-orientated episodic record (OOE-R) hypothesis, which assumes that both digits and irrelevant sounds are represented as potentially competing paths on a multidimensional surface. The OOE-R hypothesis is not spelled out in detail but would appear to assume that serial order is based on chaining, whereby each item acts as a stimulus for the response that follows, which in turn acts as a further stimulus.

Retaining Serial Order

A typical memory span is around six or seven digits, not because the digits themselves are forgotten, but rather because their order is lost. Retaining serial order is a crucial demand for a wide range of activities, notably including language, in which sequences of sounds within words and words within sentences must be maintained, and skilled motor performance such as striking a ball with a bat or playing the piano. However, as Lashley (1951) points out, it is far from easy to explain how this is achieved. The most obvious hypothesis is through the previously described mechanism of chaining through sequential associations. However, this has some major potential problems; if one item is lost, then the chain is broken and subsequent recall should fail, and yet it is often the case that despite errors in the middle of a sequence, the latter part is reproduced correctly. Similarly, if an item is repeated within the chain (e.g., *7 5 3 5 9 6*), then the chain should be disrupted, but this disruption, when it occurs, is typically far from dramatic.

A third phenomenon appears to be even more problematic. This again is an effect that was discovered when trying to solve a practical problem, that of trying to reduce the negative impact of phonological similarity on the recall of postal codes. It seemed plausible to me to assume that the principal effect of similarity would come from having two or more similar items bunched together, in which case it might prove possible to greatly minimize the effect by alternating similar and dissimilar items (e.g., *dfvkpl*). The results were disappointing; the similar items appeared to be just as liable to be forgotten when sandwiched between dissimilar items as when they were adjacent, so we put the experiment to one side. It was only later, when I was attempting to pin down the nature of the phonological loop effect, that I realized that our result had clear implications for theories of serial order retrieval in general (Baddeley 1968) and were in particular inconsistent with hypotheses that depended upon chaining. The argument goes as follows: If one considers a sequence of six letters as a series of pairs, then we know that the principal source of interference comes from similarity at the stimulus level, which then gives rise to errors on the subsequent response (Osgood 1949). We would therefore expect errors to follow the similar items, whereas in fact the similar items themselves were the main source of error (Baddeley 1968). This result has continued to present a challenge to models of serial order.

The past decade has seen considerable activity in the attempt to produce clearly specified computational or mathematical models of serial order retention, with a number located within the phonological loop tradition. Very briefly, approaches fall into two categories. One class of models assumes that items are associated with a series of internal markers, which may be temporal oscillators as in Brown et al.'s (2000) OSCAR hypothesis, or other forms of ordinal marking, as in the case of the model and its subsequent refinement by Burgess & Hitch (1999, 2006). A second approach is typified by the primacy hypothesis of Page & Norris (1998), which assumes a limited capacity of excitation that is shared among the sequence of items. The first item is the most strongly activated, the second slightly less, and so forth. At recall, the strongest item is retrieved first and then inhibited to avoid further repetition before going on to the next strongest. Both of these approaches can handle the similarity sandwich effect, as they do not depend upon chaining. Furthermore, they require two stages, a store and a serial order link, offering an interpretation of the irrelevant sound effect in terms of adding noise to this additional stage (Page & Norris 2003), an explanation as to why similarity between irrelevant and remembered items is not important.

Modeling serial order continues to be a very lively field with considerable interaction between proponents of the different models, which are now starting to become more ambitious. Burgess and Hitch are now attempting to model the link between the phonological loop and long-term phonological learning (Burgess & Hitch 2006, Hitch et al. 2009), while a further challenge being addressed lies in the interpretation of chunking, the effect that makes sentences so much more readily recalled than scrambled words (Baddeley et al. 2009). Can models of serial order in verbal STM be generalized to visual STM? The answer seems to be that they can (Hurlstone 2010). If so, do they reflect a single common system? I myself think it more likely that evolution has applied the same solution to a problem, maintaining serial order, that crops up in a range of different domains.

The Phonological Loop and LTM

What function might the phonological loop (PL) serve, other than making telephoning easier (an unlikely target for Mother Nature)? The opportunity to investigate this question cropped up when an Italian colleague, Giuseppe Vallar, invited me to help him to investigate a patient, PV, with a very pure and specific deficit in phonological STM. Her intellect was preserved, but her auditory digit span was only two items. She had fluent language production and comprehension, except for long, highly artificial sentences in which ambiguity could only be resolved by retaining the initial part of a long sentence until the end, again not a great evolutionary gain. We then came up with the idea that her phonological loop might be necessary for new long-term phonological learning. We tested this by requiring her to learn Russian vocabulary (e.g., *flower-svieti*), comparing this with her capacity for learning to pair unrelated Italian words, for example (*castle-table*). When compared to a group of matched controls, her capacity to learn native language pairs was normal, whereas she failed to learn a single Russian word after ten successive trials, a point at which all the normal participants had perfect performance (Baddeley et al. 1988). We had found a function for the phonological loop.

Although the work with PV had a major influence on my theoretical views, of much greater practical importance was my collaboration with Susan Gathercole, in which we explored the role of the phonological loop in vocabulary learning, both in children with specific language impairment and in normal children. A series of studies showed that WM plays a significant role in the initial stages of vocabulary acquisition and is also linked to reading skills (see

Baddeley et al. 1998 for a review). It formed the basis of an extensive and successful application of the M-WM theory to the identification and treatment of WM deficits in school-age children (Gathercole & Alloway 2008; Gathercole et al. 2004a,b).

At a theoretical level, work with PV led to a major development. I had previously tended to treat WM and LTM as separate though interrelated systems. The fact that the loop specifically facilitates new phonological learning implies a direct link from the loop to LTM. Gathercole (1995) showed that existing language habits influence immediate nonword recall, making the nonwords that have a similar letter structure to English, such as *contramponist*, easier than less familiar sounding nonwords such as *loddenapish* (Gathercole 1995). This suggests that information flows from LTM to the loop, as well as the reverse. Furthermore, it seemed reasonable to assume that a similar state of affairs would occur for the visuo-spatial sketchpad, leading to a revision of the original model along the lines indicated in **Figure 2**. Here, a crucial distinction is made between WM, represented by a series of fluid systems that require only temporary activation, and LTM, representing more permanent crystallized skills and knowledge.

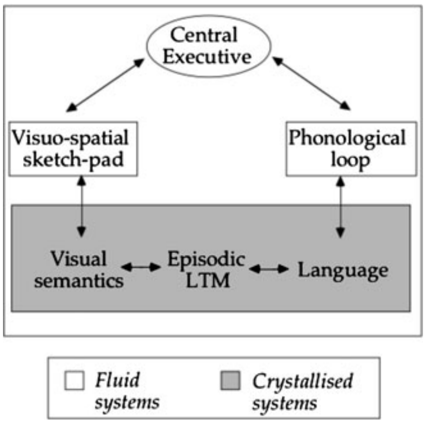

Figure 2

A modification of the original model to take account of the evidence of links between working memory and long-term memory (LTM).

The Phonological Loop: Master or Slave?

In formulating our model, we referred to the loop and sketchpad as slave systems, borrowing the term from control engineering. It is, however, becoming increasingly clear that the loop can also provide a means of action control. In my own case, this first became obvious during a series of studies of the CE, in this case concentrating on its capacity for task switching. We used a very simple task in which participants were given a column of single digits and required in one condition to add 1 and write down a total, and in another condition, to subtract 1, or in the switching condition, to alternate addition and subtraction. Switching leads to a substantial slowing, and we wanted to know why. We used dual task methods, disrupting the CE with an attentionally demanding verbal task and a task involving simple verbal repetition. To our surprise, switching was disrupted almost as much by articulatory suppression as by the much more demanding executive task. It became clear that people were using a simple subvocal code of "plus-minus-plus," etc., to cue their responses. When the relevant plus and minus signs were provided on the response sheet, the suppression effect disappeared (Baddeley et al. 2001). Similar results have been obtained and further developed by Emerson & Miyake (2003).

The importance of self-instruction had of course already been beautifully demonstrated by the great Russian psychologist Alexander Luria, who showed that children gradually learn to control their actions using overt self-instruction, a process that later becomes subvocal. He went on to demonstrate the value of self-instructions in neuropsychological rehabilitation (Luria 1959).

The Phonological Loop: Critique

The loop is probably the best-developed and most widely investigated component of WM, possibly because of the availability of a few simple tools such as the phonological similarity, word length, and suppression effects. It is, however, only one very limited component of WM. When its use in digit span is prevented by combining visual presentation with articulatory suppression, the cost is something in the region of two digits (Larsen & Baddeley 2003). Its strength is that it can provide temporary sequential storage, using a process that is rapid, and requires minimal attention. It is a system that is extremely useful, widespread, and one that, as experimenters, we ignore at our peril. The analogy that comes to mind is that of the role of the thumb in our motor behavior: small, not essential, but very useful. There is, however, a danger of exaggerating its importance. It appears to be this that Nairne criticized under the label "the standard hypothesis" (Nairne 2002), by which he appears to refer to attempts to account for a range of time-specified STM effects purely in terms of the loop. This hypothesis seemed to be attributed to myself, although as discussed elsewhere (Baddeley 2007, pp. 35–38), Nairne's criticisms do not apply to WM more generally. I agree that what Nairne describes as the standard hypothesis is far from adequate as a theory of WM or even as a general account of STM.

I have discussed the phonological loop thus far as if it were limited to the storage of heard and spoken speech. It is important to note, however, that the same system, operating under broadly similar constraints, appears to underpin memory for both lip-read and signed material (see Rönnberg et al. 2004 for a review). All of these are language related, which raises the question of whether the same system is used for nonlinguistic auditory information such as environmental sounds and music. Neither of these topics is well explored, although there is growing interest in comparing language and music and some indication of overlap (Williamson et al. 2010).

VISUO-SPATIAL SKETCHPAD

Interest in visuo-spatial memory developed during the 1960s, when Posner & Konick (1966) showed that memory for a point on a line was well retained over a period ranging up to 30 seconds, but it was disrupted by an interpolated information-processing task, suggesting some form of active rehearsal. Dale (1973) obtained a similar result for remembering a point located in an open field. In contrast to these spatial memory tasks, Posner & Keele (1967) produced evidence suggesting a visual store lasting for only two seconds. However, their method was based on speed of processing letters, in which a visual letter code appeared to be superseded by a phonological code after two seconds. Although this could reflect the duration of the visual trace, it could equally well reflect a more slowly developing phonological code that then overrides the visual.

Visual STM

A colleague, Bill Phillips, and I decided to test this using material that would not be readily nameable. We chose 5 × 5 matrices in which approximately half the cells would be filled at random on any given trial. We tested retention over intervals ranging from 0.3 to 9 seconds, by presenting either an identical stimulus or one in which a single cell was changed, with participants making a same/different judgment. We found a steady decline over time, regardless of whether we measured performance in terms of accuracy or reaction time (Phillips & Baddeley 1971). A range of studies by Kroll et al. (1970), using articulatory suppression to disrupt the use of a name code in letter judgments, came to a similar conclusion, that the Posner and Keele result was based on switching from a visual to a phonological code, perhaps because of easier maintenance by subvocal rehearsal. Meanwhile, Phillips went on to investigate the visual memory store using matrix stimuli, demonstrating that accuracy declines systematically with number of cells to be remembered (Phillips 1974), suggesting limited visual STM capacity. It was this work that influenced our initial concept of the visuo-spatial sketchpad.

Spatial STM

The most frequently used clinical test of visuo-spatial memory is the Corsi block-tapping test (Milner 1971), which is spatially based and involves sequential presentation and recall.

The participant views an array of nine blocks scattered across a test board. The tester taps a sequence of blocks, and the participant attempts to imitate this. The number of blocks tapped is increased until performance breaks down, with Corsi span typically being around five, about two less than digit span. Della Sala et al. (1999), using a modified version of the Phillips matrix task, showed that visual pattern span is dissociable from spatial Corsi span, with some patients being impaired on one while the other is preserved, and vice versa. Furthermore, pattern span can be disrupted by concurrent visual processing, whereas Corsi span is more susceptible to spatial disruption (Della Sala et al. 1999). I return to the visual-spatial distinction at a later point.

Visuo-Spatial WM

During the 1970s, research moved from visual STM to its role in visual imagery. Our own studies used a technique developed by Brooks (1968), in which participants are required to remember and repeat back a sequence of spoken sentences. In half of the cases the sentences can be encoded as a path through a visually presented matrix. The other half of the instructions were not readily encodable spatially. We found that recall of the visuo-spatially codable sentences was differentially disrupted by pursuit tracking (Baddeley et al. 1975a). We interpreted this result in terms of the sketchpad, leading to the question of whether the underlying store was visual or spatial. This we tested using a task in which blindfolded participants tracked a sound source (spatial but not visual) or detected the brightening of their visual field (visual but not spatial), again while performing the Brooks task. We found that the tracking still disrupted the spatial but did not interfere with the verbal task, whereas the brightness judgment showed a slight tendency in the opposite direction, leading us to conclude that the system was spatial rather than visual (Baddeley & Lieberman 1980).

Although these results convinced me that the system was essentially spatial, Robert Logie, who was working with me at the time, disagreed and set out to show that I was wrong. He succeeded, demonstrating that some imagery tasks were visual rather than spatial. He used a visual imagery mnemonic whereby two unrelated items are associated by forming an image of them interacting; for example, *cow* and *chair* could be remembered as a cow sitting on a chair. Logie (1986) showed that this process can be disrupted by visual stimuli such as irrelevant line drawings or indeed by simple patches of color. There are now multiple demonstrations of the dissociation of visual and spatial WM. Klauer & Zhao (2004) critically review this literature before performing a very thorough series of investigations controlling for potential artifacts; their results support the distinction between visual and spatial STM, a distinction that is also supported by neuroimaging evidence (Smith & Jonides 1997).

Yet further fractionation of the sketchpad seems likely. Research by Smyth and colleagues has suggested a kinesthetic or movement-based system used in gesture and dance (Smyth & Pendleton 1990). Another possible channel of information into the sketchpad comes from haptic coding as used in grasping and holding objects, which in turn is likely to involve a tactile component. Touch itself depends on a number of different receptor cells capable of detecting pressure, vibration, heat, cold, and pain. We currently know very little about these aspects of STM, and my assumption that information from all of these sources converges on the sketchpad is far from clearly established.

The nature of rehearsal in the sketchpad is also uncertain. Logie (1995, 2011) suggests a distinction between a "visual cache," a temporary visual store, and a spatial manipulation and rehearsal system, the "inner scribe," although the precise nature of visuo-spatial rehearsal remains unclear.

THE CENTRAL EXECUTIVE

The Executive as Homunculus

The CE is the most complex component of WM. Within the original model it was assumed to be capable of attentional focus, storage, and decision making, virtually a homunculus, a little man in the head, capable of doing all the clever things that were outside the competence of the two subsystems. Although our model tended to be criticized for taking this approach, like Attneave (1960) I regard homunculi as potentially useful if used appropriately. It is important that they are not seen as providing an explanation, but rather as a marker of issues requiring explanation. Provided the various jobs performed by the homunculus are identified, they can be tackled one at a time, hopefully in due course allowing the homunculus to be pensioned off.

Much of our work has used concurrent tasks to disrupt the various components of WM, with the assumption typically being that attentionally demanding tasks will place specific demands on the CE, in contrast to tasks that require simple maintenance. For example, counting backward in threes from a number such as 271 is assumed to load the executive, whereas simply repeating 271 would not. This and related tasks have proved to be a successful strategy for separating out contributions of the three initially proposed WM subcomponents (e.g., Baddeley et al. 2011).

Fractionating the Executive

In an attempt to specify the functions of the CE, I speculated as to what these might be; what would any adequate executive need to be able to do? I came up with four suggestions (Baddeley 1996). First it would need to be able to focus attention; evidence of this came from the impact of reducing attention on complex tasks such as chess (Robbins et al. 1996). A second desirable characteristic would be the capacity to divide attention between two important targets or stimulus streams. I had been studying this in collaboration with Italian colleagues for a number of years, focusing on Alzheimer's disease. We selected two tasks involving separate modalities: one verbal, involving recall of digit sequences, and the other requiring visuospatial tracking. We titrated the level of difficulty for each of these to a point at which our patients were performing at the same level as both young and elderly controls. We then required tracking and digit recall to operate simultaneously. There was a marked deficit in the performance of the patients when compared to either of the two control groups. Perhaps surprisingly, age did not disrupt this specific executive capacity, provided the level of difficulty is equated in the first place (Logie et al. 2004). In the absence of titration of level of difficulty, however, performance tends to decline with age on the tasks when performed singly, with the deficit even greater when the two tasks are performed at the same time (Riby et al. 2004).

The third executive capacity we investigated involved switching between tasks, for which we felt there might be a specific control system. As mentioned earlier, we chose to study a task involving alternating between simple addition and subtraction, using a demanding concurrent verbal executive task and articulatory suppression as its nondemanding equivalent. We found a large effect of articulatory suppression coupled with a rather small additional effect when an executive load accompanied suppression. The study of task switching has expanded very substantially in recent years (Monsell 2005), becoming theoretically rather complex, and in my view at least, arguing against a unitary executive capacity for task switching. I should point out that there are many other suggestions as to the basic set of executive capacities that are too numerous to discuss in this context (see, for example, Engle & Kane 2004, Miyake et al. 2000, Shallice 2002).

Interfacing with LTM

The fourth executive task that I assigned to our homunculus was the capacity to interface with LTM. In an attempt to constrain our WM model, we had made the assumption that the CE was a purely attentional system with no storage capacity (Baddeley & Logie 1999). However, this created a number of problems. One concerned the question of how subsystems using different codes could be integrated without some form of common storage. Participants do not simply use either one code or another, but rather combine them, with both visual and phonological codes being usable simultaneously (Logie et al. 2000). This capacity is particularly marked in the case of language processing, where a single phrase can show the influence of phonological coding at short delays and semantic coding at longer intervals (Baddeley & Ecob 1970). Memory span for unrelated words is around 5, increasing to 15 when the words make up a sentence. This enhanced span for sentence-based sequences seems to reflect an interaction between phonological and semantic systems rather than a simple additive effect (Baddeley et al. 1987), a conclusion that is consistent with later dual-task studies (Baddeley et al. 2009). But how might this interaction occur?

A further challenge to the concept of a purely attentional executive came from the very extensive work on individual differences in WM stemming from the initial demonstration by Daneman & Carpenter (1980) of a correlation between a measure they termed "WM span" and capacity for prose comprehension. Their measure required participants to read out a sequence of sentences and then recall the final word of each. This and similar tests that require the combination of temporary storage and processing have proved enormously successful in predicting performance on cognitive tasks ranging from comprehension to complex reasoning and from learning a programming language to resisting distraction (see Daneman & Merikle 1996 and Engle et al. 1999 for reviews). Such results were gratifying in demonstrating the practical significance of WM, but embarrassing for a model that had no potential for storage other than the limited capacities of the visuo-spatial and phonological subsystems. In response to these and related issues, I decided to add a fourth component, the episodic buffer (Baddeley 2000). Although I was reluctant to add further systems to the multicomponent theory, I felt that one in 25 years was perhaps acceptable.

THE EPISODIC BUFFER

The characteristics of the new system are indicated by its name; it is episodic in that it is assumed to hold integrated episodes or chunks in a multidimensional code. In doing so, it acts as a buffer store, not only between the components of WM, but also linking WM to perception and LTM. It is able to do this because it can hold multidimensional representations, but like most buffer stores it has a limited capacity. On this point we agree with Cowan (2005) in assuming a capacity in the region of four chunks. I made the further assumption that retrieval from the buffer occurred through conscious awareness, providing a link with our earlier research on the vividness of visual and auditory imagery (Baddeley & Andrade 2000). This results in a theory of consciousness that resembles that proposed by Baars (1988), which assumes that consciousness serves as a mechanism for binding stimulus features into perceived objects. He uses the metaphor of a stage on which the products of preconscious processes, the actors, become available to conscious awareness, the audience.

Our new component could be regarded as a fractionation of our initial 1974 version of the CE into separate attentional and storage systems. It had a number of advantages in addition to providing a possible answer to the question of the interaction between LTM and WM. At a theoretical level it formed a bridge between our own bottom-up approach based on attempting to understand the peripheral systems first, and the more top-down approaches predominant in North America, which were more concerned with analyzing the executive and attentional aspects of WM (e.g., Cowan 2005, Engle et al. 1999). Perhaps for this reason, the concept appears to have been welcomed and is frequently cited. However, although that suggests that people find it useful, if it is to be theoretically productive, there is a need to use it to ask interesting and tractable questions, a challenge that has kept Graham Hitch, Richard Allen, and myself busy over recent years.

WM and Binding

Like Baars (1988), we assume that a central role of the buffer is to provide a multidimensional

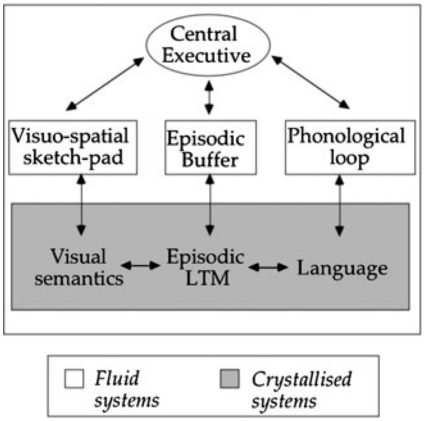

Figure 3

The model following the introduction of a fourth component, the episodic buffer, a system for integrating information from a range of sources into a multidimensional code (Baddeley 2000).

medium that allows features from different sources to be bound into chunks or episodes, not only perceptually but also creatively, allowing us to imagine something new, for example, an ice-hockey-playing elephant. We could then reflect on this new concept and decide, for example, whether our elephant would be better doing a mean defensive body check or keeping goal. This all seemed likely to be an attention-demanding process, so we speculated that the buffer would depend heavily on the CE. In the initial (Baddeley 2000) model (see **Figure 3**), I intentionally required all access to go through the executive, arguing that we could then investigate empirically whether other links were needed.

We studied the role in binding played by each of the three initial components of WM, using our well-tried concurrent task strategy to disrupt each in turn. If, as our initial hypothesis proposed, the CE controls access to and from the buffer, then an attentionally demanding concurrent task should have a very substantial effect on the capacity to bind information, in contrast to minor effects from disrupting the other subsystems. We decided to examine binding in two very different modalities, namely the binding of visual features into perceived objects on one hand, and the binding of words into sentences on the other.

Visual Binding and WM

Our work on visual binding was strongly influenced by some new developments that were beginning to extend the methods applied to the study of visual attention to the subsequent short-term storage of perceived items. A central question of this approach concerned the factors that determine the conditions under which features such as color and shape are integrated and bound into perceived and remembered objects. The basic experimental paradigm was developed by Luck & Vogel (1997, Vogel et al. 2001). As in the work of Phillips (1974), it involved presenting an array of visual stimuli, followed (after a brief delay) by a probe stimulus, with participants deciding whether or not the probe had been in the array. A number of important results emerged, notably including the observation that capacity was limited to about four objects and was approximately the same, regardless of whether participants were remembering only a single feature, for example, color or shape, or were required to bind the two features and remember not only that a red stimulus had been presented, or a square, but also that the two had been bound together as a red square (Vogel et al. 2001). A subsequent study by Wheeler & Treisman (2002) obtained the same result when testing involved a single probe item. However, they found a binding impairment when the memory test required searching through an array of stimuli in order to find a target match, a result they interpreted as suggesting that maintaining the binding of features was attentionally demanding.

We ourselves tested the attentional hypothesis using our concurrent task procedure. Presentation of the stimulus array was accompanied by a demanding task such as counting backward by threes. If the CE is heavily involved in binding, then the concurrent task should prove more detrimental to the binding condition (e.g., remembering a red square) than to either of the single-feature probe tasks (e.g., red or square). We compared the backward counting condition to one involving articulatory suppression. As expected, we found an overall impairment in performance when accompanied by backward counting. However, this was just as great for the single features as for the binding condition.

A series of further studies explored this finding, using other concurrent tasks and more demanding binding conditions. In one case, for example, shapes and the color patches to which each shape should be bound were presented in separate locations. In another study, the features to be bound were separated in time, while a third experiment presented one feature visually (e.g., a patch of red) and the associated shape verbally. Although some of these activities led to a lower overall level of performance, in no case did we obtain a differential disruption of binding (see Baddeley et al. 2011 for a review).

The final experiment of the Allen et al. (2006) paper did, however, obtain a differential effect. In this study, colored shapes were presented sequentially, followed by a probe. When the final item was probed, the results were as before: no additional binding deficit. However, earlier items did show poorer retention of bound stimuli. We interpreted this as suggesting that binding did not demand extra attention but that maintaining it against distraction did. We explored this disruption effect further, again using simultaneous presentation, but this time inserting a single additional item that participants were instructed to ignore between presentation and test. Binding was differentially impaired even though participants were told to ignore the suffix, which suggests that although visual binding per se is not attention demanding, maintaining bindings against distraction is (see Baddeley et al. 2011 for an overview).

Binding in Verbal WM

Although it appears that attention may be useful for maintaining visual bindings, our data indicate that the simple binding of color and shape is not itself attention demanding. It could, of course, be argued that perceptual binding is atypical in not requiring central resources. Fortunately, however, as part of our converging operations approach to theory, we had pursued a parallel series of experiments investigating the role of executive processes in the binding of words into chunks during retention of spoken sentences.

We carried out a series of experiments, the results of which can be summarized quite simply (Baddeley et al. 2009). Concurrent tasks involving the visuo-spatial sketchpad had a small but significant effect on recall that increased when they also had a visually based executive component. Simple articulatory suppression had a greater effect that was further amplified when both suppression and attentional load were required. Most importantly, however, none of these tasks differentially disrupted the binding of words into chunks as reflected in magnitude of the advantage in recalling sentences over unrelated word sequences. Hence, just as with visual binding, although concurrent tasks impair overall performance, they do not appear to interfere with the binding process itself, which in the case of sentences, we assume operates relatively automatically in LTM.

The evidence from both visual and verbal binding is thus inconsistent with the original proposal that the process of binding involves the active manipulation of information within the episodic buffer, which we now regard as being an important but essentially passive structure on which bindings achieved elsewhere can be displayed. It remains important in that it allows executive processes to carry out further manipulation. This may in turn lead to further bindings involving, for example, the binding of phrases into integrated sentences or objects into complex scenes.

In conclusion, although binding is sometimes discussed as if it were a unitary function, we suggest that it differs depending on the specific type of binding involved. For example, binding may be perceptual or linguistic, and it may be temporary, as required to perform WM tasks, or durable, as in the binding of new information to its context in LTM, a capacity that is disrupted in amnesic patients, who may nonetheless show normal binding in WM (Baddeley et al. 2010). All of these types of binding may, however, result in bound representations accessible through the episodic buffer.

LINKING LONG-TERM AND WORKING MEMORY

Is WM Just Activated LTM?

A number of approaches describe WM as activated LTM (e.g., Cowan 2005, Ruchkin et al. 2003). My view on this issue is that working memory involves the activation of many areas of the brain that involve LTM. This is also true of language, for which activated LTM is not taken as an explanation. I assume that in the case of Cowan's (2005) model, it is a way of referring to those aspects of WM that are not his current principal concern and not a denial of a need for further explanation. He and I would, I think, agree that the phonological loop, the simplest component of WM, is likely to depend on phonological and lexical representations within LTM as well as procedurally based language habits for rehearsal.

Long-Term WM

Ericsson & Kintsch (1995) proposed this concept in explaining the superior performance of expert mnemonists, going on to extend it to the use of semantic and linguistic knowledge to boost memory performance. They argue that these and other situations utilize previously developed structures in LTM as a means of boosting WM performance. I agree, but I cannot see any advantage in treating this as a different kind of WM rather than a particularly clear example of the way in which WM and LTM interact.

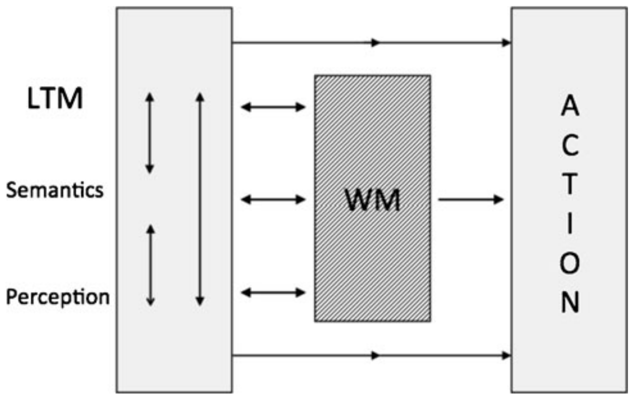

Figure 4

My current view of the complex and multiple links between working memory (WM) and long-term memory (LTM).

LTM and the Multicomponent Model

It seems likely that some of the misunderstandings confronting M-WM stem from the rather limited links with LTM shown in **Figures 2** and **3**. This was also reflected in a disagreement between myself and Robert Logie, who insisted that all information entered the sketchpad via LTM. It was only when I tried to represent my views in the simple model shown in **Figure 4** that we found we agreed. Incoming information is processed by systems that themselves are influenced by LTM. I see WM as a complex interactive system that is able to provide an interface between cognition and action, an interface that is capable of handling information in a range of modalities and stages of processing.

NEUROBIOLOGICAL APPROACHES TO WORKING MEMORY

The development of my own views on WM has been strongly influenced by the study of patients with neuropsychological deficits, and particularly by patients with specific impairment in the absence of general cognitive deficits. Brain damage can be seen scientifically as producing a series of unfortunate experiments of nature. Nature is not usually a good experimenter: Patients typically have a range of different deficits, but just occasionally "pure" deficits occur that potentially, given careful and thorough investigation, allow clear theoretical conclusions to be drawn. These can then be extended to help diagnose and treat patients with related but more complex disabilities.

There are two aspects of such research: the behavioral, linking the performance of the patient to cognitive psychology, and the neurobiological, linking it to its anatomical and neurophysiological basis. Both are important and will ultimately be combined. However, my own expertise and current concern is for the behavioral and the extent to which neurobiological research has so far contributed to the cognitive understanding of M-WM.

There is no doubt that the popularity of the concept of WM owes a great deal to neurobiological studies that appear to suggest that WM may depend on one or more specific anatomical locations. Single-unit recording of the brains of awake monkeys performing a visual STM task found that continued activation of cells in the frontal cortex was associated with successful recall, and disrupted activation was associated with failure (Goldman-Rakic 1988). This led some to conclude that it reflected a specific frontal location of WM. My own view was that it probably formed part of a complex circuit underpinning the visuo-spatial sketchpad. Subsequent discovery of similar cells elsewhere in the brain is consistent with this view.

A second source of apparent support for the concept of WM came from neuroimaging when the various subsystems of M-WM appeared to be relatively closely localizable (for reviews, see Henson 2001, Smith & Jonides 1997). This led to a large number of further neuroimaging investigations from many different laboratories, producing a range of different results, which when fed into a meta-analysis often failed to show a consistent pattern (see Baddeley 2007, chapter 7). My own view is that this simply reflects the unreliability of such results and the complexity of WM, as well as the need to modify paradigms to fit the constraints of neuroimaging, for example, avoiding overt speech.

A different interpretation is offered by Jonides et al. (2008), who comment on the "near-revolutionary changes in psychological theories about STM, with similarly great advances in the neurosciences" that have occurred in the past decade. Their very ambitious interpretation of the "mind and brain of STM" is discussed below. More generally, although I think it is very important to understand the neurobiological basis of WM, I am not yet convinced that it has made a major contribution to psychological theories of WM. This does not reflect a general rejection of neuroimaging, which offers an essential and potentially powerful tool for understanding cognition and its neural basis. There are some important areas, such as those investigating conscious awareness, in which neuroimaging provides a crucial component in testing and potentially validating distinctions such as that between "remembering" and "knowing" (Düzel et al. 2001). In the case of WM, however, I have two major sources of doubt. The first concerns the lack of apparent replicability in the field. The second more basic concern is the validity of the assumption that anatomical localization will provide a firm theoretical basis for a system as complex as WM in the absence of a much better understanding of the temporal structure of activation than is typically available at present.

SOME ALTERNATIVE APPROACHES

Theories of STM

A good deal of the controversy surrounding M-WM concerns those aspects associated with STM rather than WM, and in particular with the phonological loop model. Whereas some are concerned with the specifics of the model, such as trace decay, for example, other concerns stem from a difference in purpose. Much of our own work has focused on analyzing one source of short-term storage, that based on phonological coding. That means using tasks that minimize semantic and other longer-term factors. Other theorists focus on the whole range of codes that may be contributing, as in the case of Nairne's (1990) feature model. These will inevitably include long-term factors such as semantic coding and will emphasize the similarities between STM and LTM. I regard these two approaches as complementary.

Theories of WM

There are a number of ambitious models of WM that I regard as broadly consistent with the multicomponent framework, although each has a different emphasis and terminology.

Cowan's Embedded Processes Theory

Cowan defines WM as "cognitive processes that are maintained in an unusually accessible state" (Cowan 1999, p. 62). His theory involves a limited-capacity attentional focus that operates across areas of activated LTM. A central issue for Cowan over recent years has been to specify the capacity of this attentional focus and hence the capacity of WM. He produces extensive evidence to suggest that, unlike an earlier suggestion of seven items, the capacity is much closer to four. Importantly, however, this is four chunks or episodes, each of which may contain more than a single item (Cowan 2005).

At a superficial level, Cowan's theories might seem to be totally different from my own. In practice, however, we agree on most issues but differ in our terminology and areas of current focus. I see Cowan's model as principally concerned, in my terminology, with the link between the CE and the episodic buffer. Cowan refers to the material on which his system works as "activated LTM" but does not treat this as providing an adequate explanation, accepting the need for a more detailed analysis of the processes operating beyond attentional focus as reflected in his extensive and influential work on verbal STM, research that interacts with and is complementary to the phonological loop hypothesis of verbal STM (e.g., Cowan et al. 1992). I regard our differences as principally ones of emphasis and terminology.

Individual Difference–Based Theories

The demonstration by Daneman & Carpenter (1980) that WM-span measures can predict comprehension has provided a major focus of research on WM over the past 30 years, involving multiple replications and extensions (Daneman & Merikle 1996). At a theoretical level, there has been considerable interest in identifying the feature of such complex span measures that allows them to predict cognitive performance so effectively. Purely correlational approaches to this issue have a number of limitations, and in my view, the most promising work in this area comes from combining experimental and correlational methods to tackle the question of why some people are better able to sustain material under these complex conditions. Some explanations focus on the capacity to utilize gaps between the processing operations of the span task in order to maintain a fading memory trace (Barrouillet et al. 2004). Others also assume the need to resist time-based decay but emphasize efficiency at switching between the various tasks involved in span (Towse et al. 2000) or they emphasize the role of interference rather than decay (Saito & Miyake 2004).

However, the most extensively developed theoretical account of the mechanisms underpinning WM capacity is that proposed by Engle and colleagues (Engle et al. 1999, Engle & Kane 2004). They emphasize the importance of inhibitory processes, which they argue are crucial to shielding the memory content from potential disruption. Much of their work involves a combination of individual difference and experimental approaches, typically initially testing a large group of participants and then selecting two subgroups, those with very high and those with very low WM span. They have demonstrated that such groups differ not only in WM performance but also in susceptibility to interference across tasks ranging from recall from episodic LTM, through the capacity to generate items from a semantic category, to performance on an antisaccade task of eye movement control (for a review, see Engle & Kane 2004). Although this is an impressive program of work, I suspect that a theory of executive processing based entirely on inhibitory control may be a little narrow. Control is clearly important, but I suspect people also differ in more positive aspects of attentional capacity.

In general, I would see most of the models of WM based on individual differences as consistent with the broad M-WM framework typically focusing on executive control but accepting the contribution of separate visual and verbal STM components (see Alloway et al. 2006 for an example of such a model). Once again, overall similarities may be obscured by terminological differences. Engle and colleagues (Unsworth & Engle 2007) have recently reverted to an earlier distinction between primary and secondary memory, which I would interpret in terms of the distinction between the fluid and crystallized systems that reflect temporary structural representations in the M-WM model (see **Figure 2**).

Jonides and the Mind and Brain of STM

This approach (Jonides et al. 2008) is strongly influenced by neuroimaging in assuming, for each of a range of modalities, that perception, STM, and LTM are all performed in the same anatomical locations. They also cite evidence from neuropsychology, suggesting that amnesic patients have a general difficulty in binding features together (Hannula et al. 2006, Olson et al. 2006). However, this evidence has been criticized on two grounds: first, that the measures used comprise both long- and short-term components (Shrager et al. 2008), and second, that the conclusions are based on spatial binding. There is strong evidence that both spatial processing and episodic LTM depend on the hippocampus. Nonspatial binding such as that of color to shape was not found to be impaired in a hippocampally compromised amnesic patient (Baddeley et al. 2010), whereas classic amnesic patients do not appear to show evidence of a WM deficit (Baddeley & Warrington 1970, Squire 2004). Furthermore, developmental amnesic patient Jon, who has greatly reduced hippocampal volume, performs well on a range of complex WM tasks (Baddeley et al. 2011).

The major source of evidence cited by Jonides et al. comes from neuroimaging, where STM tasks often activate areas of the brain that also are involved in LTM (e.g., Ruchkin et al. 2003). However, as Jonides et al. note in their discussion of the single-unit studies, the fact that an area becomes active during a given task does not mean that it is essential for performance on that task. Presenting a word is likely to activate regions responsible for its phonological, articulatory, lexical, and semantic dimensions, but that does not mean that all these are necessary in order to repeat that word. Potentially more powerful evidence exists based on lesions in neuropsychological patients (Olson et al. 2006), but the interpretation of this has been questioned (Baddeley et al. 2010); the classic neuropsychological literature typically reports a dissociation between perceptual and memory deficits (Shallice 1988).

The "mind and brain" model proposed by Jonides et al. is somewhat complex, involving five psychological assumptions and six assumptions about neurobiological processing levels. They go on to illustrate their model, using the case of remembering three visual items over a two-second delay, resulting in a figure that involves 13 psychological processes operating across 10 neural levels. I remain somewhat skeptical as to how productive such a model will prove to be. This reflects a difference between us in theoretical style, with my own preference for the gradual development of detailed modes within a broad theoretical framework, whereas Jonides et al. are rather more ambitious.

Computational Models of WM

The WM theorists discussed so far have all taken a broad-based approach to theory. There are, however, theorists who attempt a much more detailed account of WM, typically accompanied by computer simulation. This is a very flexible approach, giving rise to a range of different models of WM, which can on occasion result in subcomponents resembling aspects of M-WM including the sketch pad (Anderson et al. 2004, p. 1037) and the loop (Anderson et al. 1996).

Barnard's (1985) ambitious computationally based "interacting cognitive subsystems" model can also be mapped directly onto M-WM. It was initially developed to account for language processing but was subsequently used extensively by Barnard to analyze situations involving human–computer interaction (Barnard 1987). The model can simulate most aspects of WM while linking it to motor control, emotion, and levels of awareness as part of a broad, ambitious, and insightful model, which in Barnard's hands has been applied with success to an impressive range of situations from choreography to theories of depression (Teasdale & Barnard 1993). However, the sheer complexity of the model makes it difficult for others to use. It is also unclear how important the computational detail really is, and indeed whether it gives an adequate account of what is happening within the more peripheral subcomponents. In discussing his attempt to produce a full simulation of the model, Howard Bowman (2011), a computer scientist who had worked with Barnard in a simulation, now advocates a hierarchical decomposition of the model using components that can be built in isolation, avoiding unnecessary detail such as premature attempts to specify at a neural level.

I suspect that undue complexity may in due course also prove to be a problem for an ambitious new model proposed by Oberauer (2010), who attempts to provide a blueprint for the whole WM system. He sees the main focus of WM as being "to serve as a blackboard for information processing on which we can construct new representations with little interference from old memories." He proposes six requirements for a WM system, namely, (*a*) maintaining structural representations by dynamic bindings, (*b*) manipulating them, (*c*) flexibly reconfiguring them, (*d*) partially decoupling these from LTM, (*e*) controlling LTM retrieval, and (*f*) encoding new structures into LTM. He postulates mechanisms for achieving each of these, hence attempting to put flesh on the previously vague concept of "activated LTM."

A crucial feature of Oberauer's model is the distinction he makes between declarative and procedural WM. Declarative WM is the aspect of WM of which we are aware, comprising most of the current work in the area, whereas procedural WM is concerned with the nondeclarative processes that underpin such operations: I assume that an example would be the process controlling subvocal rehearsal. However, he also considers a higher level of procedural control through what he refers to as the "bridge," as in the bridge of a ship, and what I myself would call the central executive. Consider the following: A participant in my experiment is instructed to press the red button when the number 1 appears, press the green for number 2, and neither for 3. We would expect this simple instruction to be followed throughout the experiment. It is as if some mini-program is set up and then runs, but we currently know very little about how this is achieved. I think the investigation of this aspect of procedural working memory, sometimes referred to as "task set," will become increasingly influential.

This is certainly a very ambitious program, and as Oberauer points out, the evidence at present is rather sparse, but it could be an exciting development. However, the sheer complexity of the model may make it difficult to evaluate experimentally. But then, I am a theoretical mapmaker and temperamentally skeptical of complex theoretical architectures. Time will tell.

WHAT NEXT?

A Speculative Model and Some Questions

I have described my attempts to turn a broad theoretical framework into a more detailed model by a process of speculation followed by empirical exploration. It is therefore perhaps appropriate to end on my own current speculations and some of the many questions they raise.

As **Figure 5** shows, my current views are not dramatically different from our original speculation, apart from the episodic buffer, and the attempt to provide considerably more speculative detail. In each case this suggests questions that will not be easily answered but that potentially offer a way forward. I consider the various components in turn.

Central executive. This is an attentional system; how does it differ from the limited-capacity component of Cowan's (2005) model?

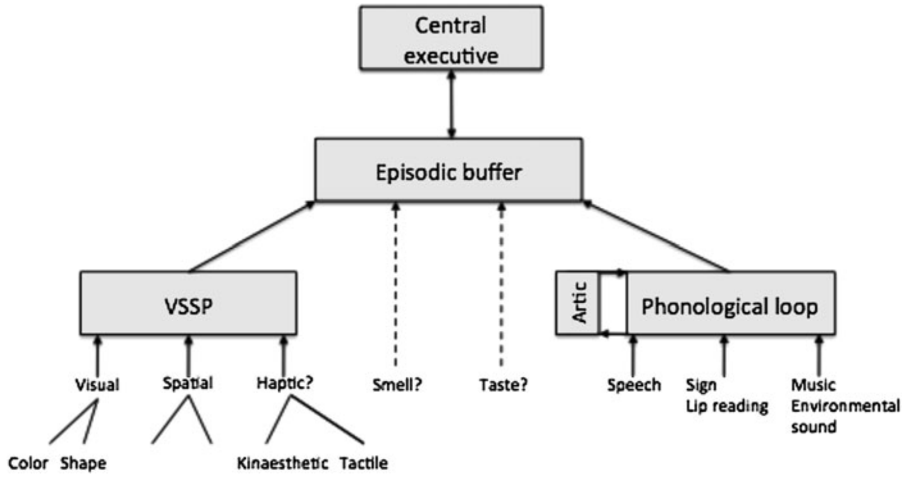

Figure 5

A speculative view of the flow of information from perception to working memory. VSSP, visuo-spatial sketchpad.

I assume that it comprises a number of executive functions, but how many, and how are they organized and interrelated? Just how far can one take attempts to explain executive control in terms of a single factor such as that of inhibition? Do we need to worry about precisely what is being inhibited and whether this differs between individuals? Do we need a concept of cognitive energy?

Episodic buffer. How should we measure its capacity? To what extent is this limited by number of chunks and to what extent by similarity between chunks? If similarity is important, can other modalities such as smell and taste be added without impacting visual or verbal capacity? Are there separate subsystems for smell and taste? How is rehearsal maintained? My current speculation is that it operates according to the principle of attentionally based refreshing, as discussed by Johnson et al. (2002). What about emotion? Elsewhere (Baddeley 2007) I have suggested that it impacts WM via a hedonic detector system; where within the M-WM system is this located?

I assume that the buffer provides access to conscious awareness; does this mean that we are not directly aware of the other subsystems but only of their products when registered in the buffer?

Phonological loop. Can we reach a conclusion on the ancient trace decay/interference controversy? Is subvocal rehearsal atypical of other types of rehearsal, as I suspect? To what extent is the loop used for remembering nonverbal material such as music or environmental sounds?

Visuo-spatial sketchpad. The visual and spatial aspects appear to be clearly separable but linked within the sketchpad; is this true of haptic, tactile, and kinesthetic memory? What is the mechanism of visuo-spatial rehearsal? Is it a spatial analogue to the phonological loop, as Logie (1995) suggests, or is it more like attentionally based refreshing? Finally, given that our attempt to link the loop with LTM through language acquisition proved very fruitful, would pursuing the link between the sketchpad and LTM prove equally useful?

Integration

Finally, how do these increasingly detailed accounts fit together to provide an interactive unitary system that mediates between perception, LTM, and action?

In Praise of Negative Results

It is of course very easy to raise questions, but much more difficult to answer them. This can lead to a program that cautiously seeks easy confirmation of what we are pretty sure we already know, resulting in confirmation bias, and an avoidance of too much risk of negative results. Negative results are a pain for a number of reasons. First of all, they are hard to interpret. They could result from a poor design or sloppy experimentation. They also raise the question of whether the experiment has sufficient power and indeed whether the question is worth asking in the first place, with all these factors making negative results harder to publish. If we are to understand WM, however, it is important to know what it does not do, and this is likely to involve negative results, as has often proved to be the case in the various stages of developing the current M-WM model. Publication is justifiably more difficult, and there needs to be a good justification for the question. Negative results can, however, be very important and publishable, provided the problem of sensitivity is addressed through inclusion of other conditions showing positive effects together with clear evidence of replication. This was the case with our original 1974 studies, where the effect of concurrent tasks was much less than anticipated, and even more so in our recent exploration of the episodic buffer (Baddeley et al. 2009, 2011).

So what does WM not do? My own conclusion after surveying the experimental literature and its implications for clinical and social psychology (Baddeley 2007) is that we have evolved an overall cognitive system that attempts to minimize the demands made on WM while allowing it to intervene where necessary. A very basic example is that of breathing, far too important to be left to working memory. However, as any diver or singer will know, we clearly do have considerable, though limited, control. Suicide by breath holding is not an option.

Applications

A central requirement of our original framework was that it should be applicable outside the laboratory. Although I have not discussed this aspect of M-WM, it does appear to have had success in achieving this, at two levels. First, through direct application of the M-WM framework to specific practical problems, Gathercole's extensive development of a WM measure applied to school-aged children has been successful in identifying children at risk and providing methods of helping teachers identify and help children with WM problems (Gathercole & Alloway 2008). Another instance is the development and validation of a dual-task performance measure for the early detection of Alzheimer's disease (Kaschel et al. 2009, Logie et al. 2004).

A second aspect of theoretical application is the use of the M-WM theory as a tool for investigating and understanding other research areas. Here the applications are very extensive, ranging from human factors to psychiatry, neuropharmacology to language therapy, and even to paleoanthropology, where the development of working memory is proposed as an explanation of the differences between Neanderthal man and homo sapiens, suggested by a study of surviving artifacts (Wynn & Coolidge 2010).

My own view is that this breadth of application has reflected the simplicity of the theoretical framework together with the availability of a few basic methodologies, such as the use of similarity effects as an indication of coding dimension and of dual-task performance as a way of controlling processing. Such techniques are easily learned, and while not guaranteeing fruitful answers, do at least provide conceptual and practical tools for investigating a wide range of problems. From a theoretical viewpoint, such practical applications can be extremely valuable both in helping explore the boundaries of the laboratory-based effects and in highlighting theoretical anomalies that have the potential to become future growing points.

CONCLUSION

So where does this leave our early question of what makes a good theory? Clearly, my own preference has been for Toulmin's view of theories as maps, coupled with the Lakatos criterion of judging success by productiveness rather than predictive accuracy. However, as we begin to fill in the empty spaces on the theoretical map, it hopefully will be increasingly possible to develop interlinked and more detailed models of the components of WM and their mode of interaction.

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